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HPB

LETTER TO THE EDITOR

Response to comments on: Alanine transaminase rather than abdominal ultrasound alone is an important investigation to justify cholecystectomy in patients presenting with acute pancreatitis

I write to thank your correspondent for his comments on our recent paper on the efficacy of alanine transaminase (ALT) in acute pancreatitis[1] and to respond to his queries. As our Materials and methods section describes, the diagnosis of biliary pancreatitis was based on radiological imaging (abdominal ultrasound [US], magnetic resonance imaging [MRI], computed tomography).[1] The definition of alcohol-induced pancreatitis provided, although not perfect, is in line with previously published definitions[2]. Our Introduction and Discussion sections explain why endoscopic US was not considered as the reference standard.[1] In response to Al-Habbal's query concerning whether or not patients in whom the initial US was negative underwent a second US, I can explain that some patients did undergo further cross-sectional imaging using MRI. However, this was not standard across the department and exact numbers cannot be given. Furthermore, if subsequent imaging showed gallstones, the patient was deemed to have had biliary pancreatitis.

Your correspondent's approach to treating patients with no obvious aetiology is interesting, but is not a strategy that is practised widely elsewhere. In the Discussion section of our paper, we allude to our belief that, in the absence of a positive US, most surgeons will not perform cholecystectomy. The aim of this paper was to highlight precisely this point: that a negative US may falsely reassure the surgeon. The tables provided allow for calculation of the probability that the underlying aetiology is biliary in origin, thereby permitting the surgeon and patient to conduct an informed discussion as to whether to proceed to cholecystectomy. However, what is the point of a second US if its results do not change the initial management strategy?

Al-Habbal's statement that abdominal US is important in assessing liver texture, the pancreatic head and the biliary tree deserves further comment. Abdominal US is particularly poor at defining the biliary tree. In addition, the pancreatic head is often not seen in acute pancreatitis for the reasons he mentions. Visualization by MRI is required to assess these factors accurately. Quite why your correspondent needs to accurately assess the biliary tree at this point is unclear as this can be done using intraoperative cholangiography. It is also known that over 80% of common duct stones will pass spontaneously in acute pancreatitis. Finally, why shouldn't an endoscopic retrograde cholangiopancreatography (ERCP) be performed without US? Your correspondent argues that he would submit a patient to cholecystectomy with a negative US, but not to ERCP.

In conclusion, I think the approach Al-Habbal describes is close to the concept raised by the paper in question: cholecystectomy should be used more widely in patients with acute pancreatitis. However, ALT has the advantage of being able to inform the patient about the probability that this will actually prevent further attacks.

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Reference

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2. Buchler MW, Freiss H, Uhl W, Malfertheiner P, eds. (2002) Chronic pancreatitis: novel concepts in biology and therapy. Blackwell Publishing, Oxford.